Programme blanc 2006

APPEL A PROJETS DE RECHERCHE

ATTENTION : Cette partie (I) sera également à saisir directement sur le site de soumission

I - FICHE D'IDENTITE DU PROJET

Référence :

(reprendre la référence qui vous sera attribuée automatiquement par le logiciel de soumission)

Titre du projet *(maximum 120 caractères)*

Theory and Application of Deep Inference

Acronyme ou titre court *(12 caractères)* *INFER*

Secteur disciplinaire principal *(cf. liste en dernière page de ce dossier)* : Sciences et technologies de l'information et de la communication (STIC)

Mots-clés (deux max. choisis dans la liste des mots-clés proposés pour ce secteur sur le logiciel de soumission)

Logique et complexité

Mots-clés libres du secteur principal (max. 4)

Deep inference, proof theory, size of proofs, proof search

Autre secteur disciplinaire secondaire[1] *(cf .note de bas de page)*:

Mots-clés (deux max. choisis dans la liste des mots-clés proposés pour ce secteur sur le logiciel de soumission)

Mots-clés libres du secteur secondaire (max. 4)

1 Un dossier ne sera effectivement évalué par un secteur secondaire que si un des participants au projet relève de ce secteur secondaire.

Résumé du projet *(maximum 5000 caractères)*

Ce projet réunit trois équipes par leur intérêt commun pour une nouvelle approche de la théorie de la démonstration, « l'inférence profonde », qui a été développée durant les cinq dernières années par un groupe de chercheurs réunis autour d'Alessio Guglielmi. Nous visons à raffiner son énorme potentiel et à l'appliquer à des problèmes liés aux fondements de la logique, ainsi qu'à des questions plus pratiques d'algorithmique des sytèmes de déduction.

Parmis les problèmes théoriques il y a le besoin fondamental d'une théorie de l'identification correcte des démonstrations et son corollaire, l'obtention d'une approche vraiment générale des réseaux de démonstration. Une autre question très voisine est l'extension de l'interprétation de Curry-Howard à ces nouvelles représentations. Parmi les problèmes pratiques nous aborderons des questions de stratégie et de complexité en recherche des preuves, en particulier dans des systèmes d'ordre supérieur. Ces questions sont reliées intimement à la formulation même de ces logiques, et le rapport évident entre l'inférence profonde et des techniques bien établies---comme la déduction modulo et l'unification sur les quantificateurs---sont des sujets que nous avons l'intention d'approfondir, étant donné leur ancrage commun dans la théorie de la réécriture. Nous voulons aussi explorer la formulation et l'utilisation de systèmes logiques plus « exotiques », de nature non-commutative, dont l'intérêt provient de l'existence d'applications, comme en liguistique et en calcul quantique. Ceci prolonge naturellement certains des premiers travaux en inférence profonde.

Il est assez évident que des résulats probants pourront obtenus pour les problèmes de la partie « pratique » de ce programme de recherche. La partie « théorique » par contre s'inscrit dans une quête qui avance pas à pas depuis des décennies maintenant, et il est difficile de faire des prédictions exacte, sauf que nous avons la certitude que notre positionnement autour de l'inférence profonde nous permettra des progrès substantiels.

Abstract *(Not exceed 5000 car.)*

This project is a grouping of three teams through their common interest for a new approach to proof theory, called « deep inference », that has been developed during the last five years by a group of researchers centered around Alessio Guglielmi. We aim at refining its enormous potential and at applying it to problems related to the foundations of logic and to more practical questions in the algorithmics of deductive systems.

Among the list of theoretical problems there is the fundamental need for a theory of correct identification of proofs, and its corollary, the development of a really general and flexible approach to proof nets. A closely related problem is the extension of the Curry-Howard isomorphism to these new representations. Among the list of more practical problems we will tackle questions of strategy and complexity in proof search, in particular for higher-order systems. These questions are intimately related to how proofs themselves are formulated in these systems, and the obvious relationship betwen deep inference and well established techniques---like deduction modulo and unification for quantifiers---are subjects that we intend to deepen, given their common grouding in rewriting theory. We also intend to explore the formuation and use of more « exotic » logical systems, for example non-commutative ones, that have interesting applications, as in linguistics and quantum computing. This is a natural continuation of some of the first results that were obtained in deep inference.

It is rather obvious that clear positive, measurable results will be obtained for some of the more « practical » parts of this research program. But the « theoretical » aspect belongs to a quest that has been seen steady progress for decades, and it is hard to make hard predictions, except that our choice of deep inference is the right way to go and that we are expecting serious progress.

Coordinateur du projet[2] (Partenaire 1)

Civilité	Nom	Prénom	Laboratoire (nom complet)	Type (établissement public, fondation, association, entreprise)
M.	Strassburger	Lutz	Laboratoire d'Informatique d'Ecole Polytechnique (LIX)	INRIA (public)

Nom des responsables scientifiques des autres partenaires

	Civilité	Nom	Prénom	Laboratoire (nom complet)	Type (établissement public, fondation, association, entreprise)
Partenaire 2	M.	Lamarche	François	Laboratoire Lorrain d'Informatique et ses Applications (LORIA)	INRIA (public)
Partenaire 3	M.	Parigot	Michel	Preuves, Programmes, Systemes (PPS)	CNRS (public)
Partenaire 4					

Nombre de personnes impliquées dans ce projet (en équivalent temps plein : ETP)[3]:

Chercheurs et enseignants-chercheurs permanents__5.175_____

Post-doctorant(s) déjà recruté(s)______ Doctorant(s) _3.9___ Ingénieurs et techniciens _______

Personnes à recruter ___2______

Durée du projet : x 36 mois ~~☐ 48 mois~~

2 Rappel : le coordinateur du projet doit consacrer au moins 30% de son temps de recherche au projet

3 Quelque soit la catégorie de personnel, il s'agit ici, pour chaque personne impliquée dans le projet, de multiplier son temps de recherche par le pourcentage de temps qu'il consacrera à ce projet.

Programme blanc 2006
B - Description du projet

Acronyme ou titre court du projet : INFER

B-1 – Objectifs et contexte : *(2 pages maximum en Arial 11, simple interligne)*
On précisera les objectifs et les enjeux en les situant dans le contexte international

The purely theoretical phase of this project addresses foundational issues in logic, issues that matter very much in computer science. These theoretical issues have a direct connection to much more practical matters, like the algorithmics of proof search and its complexity, and we will describe some of these. Everything in the end is connected with the very difficult, real-world problem of producing reliable software.

In what follows, when we write « mathematical logic », we will basically mean « proof theory », even if some other areas like model theory and set theory are sometimes concerned.

Mathematical logic has been around for more than a hundred years, but in some ways it is still not really mature, in the sense that it still lacks coherent theoretical foundations: a general language to express formal systems and the problems that are naturally associated to them, and a general methodology to suggest solutions and express them. This is sharp contrast with, say, algebraic topology, which was born at about the same time as logic, but for which the foundational issues, and the solutions that have been proposed to them, have had tremendous conceptual influence on 20th century mathematics. The rapid expansion of logic in computer science makes this need for conceptual foundations even more pressing. Deep results are being proved, but they are becoming more and more technical. The outcome is that researchers become more and more specialized, and some duplication of effort happens in related fields, due to the fact that people don't understand what their neighbors are doing anymore. And the virtuosity which is sometimes needed for proving of some of these results seems sometimes unnecessary, seemingly having to do more with artifacts of the chosen formalism than with inherent difficulty. Researchers have been aware of this for a long time, and the two most important advances in that direction for logic in the last half decade have been category theory and linear logic.

Lambek and Lawvere remarked more than forty years ago that important constructs in logic can be formulated in terms of category-theoretical concepts like adjunction. This has made many aspects of logic more algebraic, but the basic category theorist's conceptual kit is not always perfectly suited to the syntactic issues associated to proof theory; sometimes there is a slight mismatch, and the abstractions provided by category theory are always some distance away from the actual stuff that proof theorists work with.

The discovery of linear logic by Girard has shown that even if adding the constraint of linearity weakens the logic itself, this constraint brings a new geometric viewpoint to concepts like formula and cut-elimination (and thus the possibility of using geometric intuition). The more traditional logics can then be recovered by new connectives. One great triumph of linear logic is the introduction of proof nets, which, when they can be used, give very synthetic presentations of proofs for the right logics. Interestingly, proof net models usually have very nice properties from the category-theoretical viewpoint.

The present research project is interested in furthering what we believe is the next important advance, one which has been around for about only five years. In general it should and will be called « deep inference », but the current, most visible formulation of its underlying principles is called the « calculus of structures ». The main development of these ideas has been accomplished by the Proof Theory Group in Dresden (Germany) led by Alessio Guglielmi. The coordinateur of this project was in the fortunate situation to participate in that development while writing his doctoral thesis.

One aspect of proof theory which has been remarkably stable during the 20th century is how deductive systems are formulated. There has not been any significant departure from Gentzen's seminal work, which introduced the sequent calculus and natural deduction in the mid-thirties (and Tableaux-systems are a variant of sequent calculus; and earlier Frege-Hilbert-style systems are still used sporadically today). For example the deductive system that are associated with linear logic are formulated in these traditional terms, the more frequent one being the sequent calculus.

Deep inference's innovation lies at this very level: how a formal system is presented. It furthers the importance of linearity by incorporating it at the level of *deduction steps* themselves. Furthermore, in the calculus of structures, inference rules do not work on the root connective of the formula (as we know it from tableaux, sequent calculus, or natural deduction) but are conducted as rewriting steps (as in ordinary term rewriting), potentially at any depth inside the formula. This why the name « deep inference » was given to the whole enterprise. From the viewpoint of structural proof theory, deep inference seems like a step back in history, because modern proof theory has been made possible only through Gentzen's idea of rigorously exploring the concept of the main connective. The dropping of this concept caused a breakdown of all proof theoretical techniques developed since then. Indeed, most of the early research on deep inference went into the development of new techniques for proving things like cut elimination.

However, the use of deep inference results in a much finer analysis of proofs than what traditional systems permit; in particular it allows the proof of more precise versions of the classical theorems of proof theory, like interpolation and Herbrand's theorem; it also gives a more synthetic view of a formal proof. Now that these « preliminary » results are established, one important goal of the present project is to move on to the next step, namely tackling problems that are directly relevant to computer science. Furthermore, the above preliminary work has led to the discovery of logics that cannot be formalized otherwise than by deep inference (as shown by Tiu), and these logics are relevant to computer science, as we will see.

We would like to emphasize that within only 5 years (since 2000) deep inference has developed from « not existing » to a very active and continuously growing field of research. There have been at least 25 publications in the area of deep inference that went through international peer reviewing, 7 in journals and 18 in conferences and there are several more that currently undergo the reviewing process. Apart from the three French laboratories mentioned in this proposal, there are now groups in Bath (UK), Bern (Switzerland), Ottawa (Canada), Stanford (USA), Canberra (Australia), and, of course, Dresden (Germany) working on deep inference. There is a webpage dedicated to deep inference at http://alessio.guglielmi.name/res/cos/index.html (maintained by Alessio Guglielmi).

Our project proposal is a grouping of three laboratories whose expertise in the field of proof theory in computation is acknowledged on a world-wide scale. We intend to benefit from the pooling of the wide-ranging competences they represent to attack and solve a certain number of important open problems in the proof theory of deep inference. As we have said we also intend to apply deep inference method in areas of computer science where proof theory already plays a crucial role. For this we will have a close collaboration with the international groups listed in the previous paragraph. We have selected 7 key representative active lines of research, the first 4 of them are about the further theoretical development of deep inference, and the last three are about applications of deep inference in computation.

1. Reduction of bureaucracy and identity of proofs
2. Size of proofs
3. Proof nets
4. Computational interpretation of logical systems
5. Deduction modulo
6. Proof search in first-order and higher-order logic
7. Non-commutative logics

They will be discussed in more detail in Section B-2.

B-2 – Description du projet et résultats attendus *: (8 pages maximum en Arial 11, simple interligne)*
L'originalité et le caractère ambitieux du projet devront être explicités. L'interdisciplinarité et l'ouverture à diverses collaborations seront à justifier en accord avec l'orientation du projet. La capacité de ou des équipes « porteuse (s) » devra être attestée par la qualification et les productions scientifiques antérieures de leurs membres. Leurs rôles dans les différentes phases du projet devront être précisés et la valeur ajoutée des collaborations entre les différentes équipes sera argumentée. On décrira le déroulement prévisionnel et les diverses phases intermédiaires ainsi que les méthodologies employées. Les moyens demandés devront être en accord avec les objectifs scientifiques du projet.

We give in the following a detailed presentation of the seven lines of research we have selected.

1. Reduction of bureaucracy and identity of proofs

As we have said in the introduction, proof theory is still in a primitive stage compared to many other mathematical theories. This can be summarized in a nutshell by saying that it is still at the stage of *presentations*---as in presentation of a group by generators and relations---since we are able to manipulate and transform proofs in various ways and prove deep theorems about them. But we have been waiting too long for logic to reach the next stage in the natural development of a mathematical theory, that of *definitions,* where for example we have a clear idea of when to identify two proof objects because we have a precise definition of what they are. J-Y Girard has coined the colorful term of *bureaucracy* to express the source of the problem: the present state of syntax is too bureaucratic, it forces us to make unneccessary distinctions, that are only artifacts of that syntax, and keep us away from the essence that we are trying to reach. Proof nets have been the one major advance in the the fight agains bureaucracy.

Finding the right definitions is, of course, of utter importance for logic and proof theory for its own sake. We can only speak of a real « theory of proofs », if we are able to identify its objects. Apart from that, the problem is of relevance not only for philosophy and mathematics, but also for computer science, because many logical systems permit a close correspondence between proofs and programs. In the view of this so-called Curry-Howard correspondence (see also point 4 below), the question of the identity of proofs becomes the question of the identity of programs, i.e., when are two programs just different presentations of the same algorithm. In other words, the fundamental proof theoretical question on the nature of proofs is closely related to the fundamental question on the nature of algorithms. In both cases the problem is finding the right formulation that is able to avoid unnecessary syntactic bureaucracy. And naturally, as have we have said, present-day theoretical computer science is suffering from strong centrifugal tendencies that have to be countered, if we want it to stay a coherent body of work.

From this viewpoint the concept of deep inference seems to be a step back in history. Due to the new freedom of applying inference rules, the number of possible trivial rule permutations explodes, compared to formalisms based on the main connective. However, the possibility of deep inference opens up the possibility of designing new formalisms that are *a priori* free from bureaucracy. There is already preliminary research on this subject by Brünnler, Lengrand, Guglielmi, and Strassburger, which identifies various kinds of bureaucracy and suggests ways how these can be avoided.

An interesting aspect of this work on classifying bureaucracies is that much of it has a natural category-theoretical translation, in terms of functoriality and naturality. This is because the relationship between categories and deep inference is very natural but different and more subtle than the standard categorical interpretation of proofs, where, given a map (morphism)

A --> B

in a category of proofs, it makes no difference in saying that it is a a proof that A implies B or that it is a deduction of B from A. For deep inference, a map

A --> B

is a *only a deduction* of A from B, which is different in general from a proof of

A => B .

This is because the standard adjunction that defines implication does not hold in general in these categories. In particular composition of deductions *cannot* be interpreted as cut-elimination (as is usual), and it is a much more primitive, simple-minded operation. This is one important illustration of the idea that deep inference provides a finer analysis of proofs: its main constituents are inherently weaker, forcing us to formulate more precisely the strength that is needed to construct a logic.

This correspondence has led Yves Guiraud (to appear in APAL) to incorporate ideas from higher-dimensional rewriting, following the work by Burroni on polygraphs (TCS 1993) and Baez and Dolan on n-categories (e.g., Advances in Mathematics 1998). In this setting, formulas are two-dimensional objects and proofs are three-dimensional maps. Consequently, transformations between proofs are objects in four dimensions. The various kinds of bureaucracy are resolved by isotopy relations. This approach not only promises a uniform approach to all kinds of bureaucracy investigated so far, it also suggests new tools for solving the problem of the identity of proofs. Their three-dimensional representation now allows one to apply topological methods that were unavailable before.

One relatively easy aspect of this research should be the obtention of a proper, general, realistic categorical axiomatization of deep inference, But the real exciting work begins when we start looking for representations of these abstract categories, i.e., "deductive proof nets" (see below). We now have evidence that it may be necessary to go from 1-dimensional complexes---graphs---which have been the norm so far for proof nets, to higher-dimensional ones.

2. Proof nets

Proof nets are abstract (graphical) presentations of proofs such that all "trivial rule permutations" are quotiented away. Ideally the notion of proof net should be independent from any syntactic formalism. But due to the almost absolute monopoly of the sequent calculus, most notions of proof nets proposed in the past were formulated in terms of their relation to the sequent calculus. Consequently we could observe features like « boxes » and explicit « contraction links ». The latter appeared not only in Girard's proof nets for linear logic but also in Robinson's proof nets for classical logic. In this kind of proof nets every link in the net corresponds to a rule application in the sequent calculus.

The concept of deep inference allows to design entirely new kinds of proof nets. Recent work by Lamarche and Strassburger (CSL 04) could extend the theory of proof nets for multiplicative linear logic to multiplicative linear logic with units. This seemingly small step---just adding the units---had for long been an open problem, and the solution was found only by consequently exploiting the new insights coming from deep inference. A proof net no longer just mimicks the seqent calculus proof tree, but rather an additional graph structure that is put on top of the formula tree (or sequent forest) of the conclusion. This naturally leads to the following open problem:

- Extending the Lamarche/Strassburger proof nets for MLL with units to larger fragments of linear logic, i.e., including the additives (in particular, the additive constants), the exponentials, and the quantifiers, without using boxes.

Also for classical logic, for which it was long believed that one cannot have symmetric proof nets with a confluent cut elimination, it was deep inference that provided the insights that led to the design of such proof nets (Lamarche and Strassburger, TLCA 05). There are now two important open problems:

- Finding (for classical logic) a notion of proof nets that is deductive, i.e., can effectively be used for doing proof seach. An important property of deductive proof nets must be that the correctness can be checked in linear time. For the classical logic proof nets by Lamarche and Strassburger this takes exponential time (in the size of the net). There is in fact a very close relationship to point 1 above: We actually hope that eventually the right bureaucracy-free formalism and the deductive proof nets will turn out to be only two different appearances of the same mathematical objects.

- Finding a category theoretical axiomatization for the proof identifications made by the proof nets. It is well understood that the notion of cartesian closed category provides the right axioms for proof identification in intuitionistic logic and that *-autonomous categories axiomatize the proof identifications for linear logic. But for classical logic, there is no commonly agreed notion of such a category. In fact, the work by Lamarche and Strassburger (LICS 05) suggests that there is a wide range of possibilities, and not just one canonical way.

3. Size of proofs

Deep inference rules have a much finer granularity than inference rules based on the main connective. One inference rule in the sequent calculus or in a tableau system usually corresponds to a sequence of inference rules in the calculus of structures. On one side, this means that proofs in the calculus of structures are longer (but only by a constant factor) than in other systems, but on the other side this means that the calculus of structures can p-simulate many other calculi that can (in general) not p-simulate each other. This includes the sequent calculus, resolution, the tableau method, Frege-Hilbert-systems, and even the truth-table method.

As a matter of fact, given this great expressivity of deep inference, we should be able to think of traditional formalisms like the sequent calculus as *subsystems* of a general theory of deep inference, and a good theory of proof identification should turn the above *simulations* into *homomorphisms*.

This uniformity allows to exhibit short (i.e., polynomial size) proofs for all tautologies that have short proofs in one of the proof systems mentioned above. The important point here is that this can be done cut-free, i.e., analytic, which is not the case for Frege-Hilbert-systems, which are usually used when the size of proofs is investigated. We can, for example, in the calculus of structures give short cut-free (i.e., analytic) proofs for classes of tautologies which do not have short cut-free proofs in the sequent calculus (but only short proofs with cut). This suggests that deep inference should become a standard tool for analysing the complexity of proofs. But so far there has been only preliminary research in this promising direction.

4. Computational interpretation of logical systems

Intuitionistic logic is the theoretical cornerstone of functional programming; this comes through the Curry-

Howard isomorphism, that establishes a correspondence between proofs in intuitionistic logic and typed functional programs (written as lambda terms). This is because the constructivist viewpoint (the ideology behind intuitionistic logic) says that, for example, a proof of A=>B is an algorithm that transforms proofs of A into proofs of B. If we think of A, B as data types, we have a functional program. This has been an important field of research for the last 25 years.

The Curry-Howard correspondence was extended to classical logic at the beginnning of the 90s, notably via the lambda-mu calculus introduced by M. Parigot, which is closely related to actual control operators in programming languages. Since then several computational interpretations of classical logic have been proved « essentially equivalent », by the means notably of linear logic and polarizations. We still have to understand the untyped versions of these classical systems (for instance the pure lambda-mu calculus) and in particular their denotational models. It can be hoped that deep inference can provide new insights in this new, pretty much uncharted, direction of research.

But the technological transfer also goes in the other direction: So far, the computational meaning of the nomalization (i.e., cut elimination) procedure of deep inference systems is not at all investigated. By now most of the commonly used logics have a presentation as deep inference system. This includes classical logic, intuitionistic logic, linear logic, as well as various modal logics. They all come with their own cut elimination method, which is in most cases based on the technique of « splitting » (due to Guglielmi, 2002). It is an important objective of this project to provide a computational interpretation, in the sense of the Curry-Howard correspondence to these systems and their cut elimination. The goal is to provide a uniform method to do this for all systems that use Guglielmi's splitting for cut elimination.

It could very well be that deep inference has introduced a completely unexpected turn to the Curry-Howard view of things, perhaps the end of its hegemony. Recently Tiu proposed a completely local deep inference system for intuitionistic logic. That means that every inference rule only needs a bounded amount of computational resources (i.e., time and space). In particular the contraction rule is applied only to atoms. (Before it was believed that this could be done only for logics with involutive negation, i.e, classical and linear logic.) It is however completely unknown what the Curry-Howard isomorphism means in this new setting.

Another example that things are seriously being shaken up by these new developments is given by the proof nets for classical logic of Lamarche and Strassburger (see point 2 above). The important question of their computational interpretation is still open. This is because, although these nets normalize perfectly, their behavior shows some surprising properties with respect to rules like contraction. One reason is that their perfect symmetry makes them much less sequential than the polarized interpretations mentioned above.

Perhaps we will end up concludiing that some logical systems are not amenable to the Curry-Howard ideology, and that something new has to be invented. Or perhaps these new examples simply open a window on the difficult problem of the relationship between functional programming and parallelism, since traditional systems are very much sequential when viewed through the Curry-Howard lens.

5. Deduction modulo

Deduction modulo (Dowek/Hardin/Kirchner) is a framework that integrates computation and deduction in a single notion of proof. Instead of having only inference rules as smallest steps within a proof, one allows also computation steps. It is particularily well-suited to study proofs and proof search methods in various theories, such as equational theories, simple type theory, set theory, stratified set theories, etc. Similar to what happens with deep inference, such a radical change in the notion of formal proof causes a breakdown of many well-established proof theoretical techniques and properties, including cut elimination.

Deduction modulo and deep inference, are concerned with very similar problems. First, both of them are deeply related with rewriting. Furthermore, in deduction modulo, the computation steps can be incorporated in the deduction inference rules, so that proofs are identified if they follow the same deductive process, independently from the computation occurring in them. This is very close to the ideas of point 1. Moreover, this leads to shorter proofs (point 3): on the one hand because computations steps can be considered as negligible compared to deduction, on the other hand because computation (blind execution) can be more direct than deduction (non-deterministic search). A good example for this is the proof of 2+2=4 : quite elaborate in the sequent calculus with, as hypothesis, some presentation of arithmetic; but trivial if one allows computation.

Deduction modulo seems to be limited by the formalism upon which it is built, i.e. the sequent calculus. Recent work by Kirchner and Burel shows that the cut elimination property can be recovered by adding computation rules following the idea of Knuth-Bendix-completion on term rewriting systems. It seems that for doing so the sequent calculus poses artificial syntactic restrictions. For example, one can consider only computation rules that rewrite atomic propositions. There is therefore no simple way to add rewrite rules corresponding to a sequent involving quantifiers. It is very clear that deep inference naturaly overcomes this difficulty due to the greater flexibility in the design of rules. However, to actually make use of this flexibility, it is first necessary to redefine the concept of deduction modulo within deep inference.

Deep inference might also help to give an exact characterization of systems in deduction modulo for which the cut elimination holds. So far, there are only sufficient or necessary conditions. It is not even clear how it depends on the used formalism (i.e., the sequent calculus). For this, a direction of research could be based on the observation that, if only terms are rewritten, the cut elimination property is equivalent to the confluence of the rewrite system (shown by Dowek). Since this does in general not hold when one rewrites propositions, one could interpret cut elimination as some sort of « confluence property » of the deep inference deductive modulo systems, property which has to be closely determined.

6. Proof search in first-order and higher-order logic

The proof-theoretical foundations for proof search in higher-order logic are provided by Gentzen's sequent calculus. For implementation, a unification procedure has been put on top of that. Usually, a unification problem is a conjunction of equations between simply-typed lambda-terms where the free variables in the equations can be universally or existentially quantified (in the first-order case they are only existentially quantified). How to deal with mixed quantification has been worked out by Miller (JSC 92).

The problem in implementing proof search is deciding at what point the unification should happen. The sequent calculus tells us to instantiate a variable in the moment the rule for the existial quantifier is applied. However, at that moment, the machine has no way to know with what the variable should be instantiated. Only later when the proof search reaches an identity axiom, this knowledge is revealed (of course, the human reasoner always has the possibility of making a clever guess).

In practical implementations this problem is usually solved by laziness. One keeps a « hole » in the proof and postpones the unification to the very end of the proof. While this clearly works well for practical purposes, it is a disaster for the theory of proofs, because there is no well defined notion of « proof object ». There are only meta-language descriptions.

It is again the concept of deep inference that can solve that problem in a very elegant way by simply postponing the application of the rule that removes the existential quantifier. This is easily possible by working inside the formula. (Recall that the sequent calculus has to remove the quantifier in order to get access to the formula inside in the first place.) This can give a proper proof theoretical treatment to the implementations.

In order to make proper use of the freedom of deep inference, it is however necessary to extend deep inference systems to logics with higher-order quantification. The work by Bruennler on first-order logic (Studia Logica 2006) shows that this can be done in a very clean way.

7. Non-commutative logics

Deep inference has already shown its ability to produce deductive systems for logics that cannot be formalized otherwise. The paradigmatic example is system BV, which is closely related---and probably equivalent to---Retore's pomset logic, and for which there is a proof that it cannot be formulated in the sequent calculus (this is due to A. Tiu, to appear in LMCS). Non-commutative logics are used in linguistics, and also in the theory of concurrency via its formulation through logic programming. Such applications need flexibility, in the sense that one has to be able to easily construct taylor-made logics, for a given application. One goal of this research project is to test deep inference's ability to provide flexible, non-standard logics that are suited for their target applications.

One specific example of this is the development of braided logics, where the word braided has the same meaning as in conformal field theory. It now seems that the pioneering work of Fleury (PhD-thesis) and Bellin/Fleury (Archive of Math. Logic 1988) could not be finalized because it was (out of necessity) bound to the sequent calculus.

But deep inference also has something to say on how such nonstandard logics are interpreted.

Blute, Ivanov, and Panangaden (Int. J. of Theoretical Phsysics 2002) proposed the notion of a quantum causal graph for describing the evolution of open quantum systems. Within these graphs the notion of locative (spacelike) slice is defined in order to capture nonlocal correlations. They propose a way to use linear logic to describe the evolution of the locative slices, but to do so, it is in certain cases necessary to update the axioms of the system during the computation. It is a surprising observation by Strassburger, that the logic BV can fulfill this task without the need of updating the axioms during the computation.

This is very surprising. Strassburger's encoding of the causal graph mimicks Guglielmi's original intuition for introducing the logic in the first place: the non-commutative connective is used to to describe the temporal relation between two events, and the two commutative connectives are used to describe entanglement (par) and non-entanglement (tensor) of two particles. But the appeal to quantum-theoretical intuition was not posited at first: it appeared as a way to solve the problem given by Blute et al.

This clearly indicates that the last word on the application of deep inference methods in this area has not been said yet. An important next step would be to find out whether deep inference methods can be used to go beyond mere locativity, i.e., make qualitative statements about entanglement, whithout having to do the calculations in Hilbert spaces.

Naturally we also intend to tackle the problem of the equivalence between BV and pomset-logic.

B-3 – Justification scientifique des moyens demandés pour chaque équipe partenaire impliquée dans le projet.

On présentera ici une justification scientifique des moyens demandés pour chacun des partenaires impliqués dans le projet, en distinguant les demandes en équipement, fonctionnement, personnels. Pour les demandes d'équipement, préciser si les achats envisagés doivent être complétés par d'autres sources de crédits, le montant et l'origine des crédits complémentaires qui seront utilisés.

Partenaire 1

Ce projet vise à faire émerger une nouvelle communauté, qui n'a pas encore vraiment pignon sur rue. Certains membres potentiels sont à l'étranger, risquent d'y rester. En tant que bénéficiaires d'un « programme blanc » nous prendrions la responsabilité de structurer globalement cette communauté, en particulier au moyen de colloques spécialisés, ouverts sur la scène internationale. Les Partenaires 1 et 2 prendont en charge chacun l'organisation d'un de ces colloques et doit bénéficier pour cette raison d'un complément budgétaire pour des participatns extérieurs au projet. Toujours dans l'objectif de structurer cette communauté les trois partenaires procéderons aussi à l'invitation pour des cours séjours de chercheurs étrangers. Naturellement tous les partenaires doivent profiter aussi de leur propre buget de voyage et de participation à ces événements. Il convient de noter, que le fait que le projet regroupe une communauté émergente signifie aussi que le nombre de participants sur chaque site, qui actuellement limité devrait croitre au cours des trois années du projet.

Partenaire 2

La spécificité du parteraire 2 est son éloignement de Paris, et la plus grande difficulté à faire venir des étudiants motivés et bien formés pour des thèses (ceci est aussi dû à la structuration du laboratoire d'accueil.) Le besoin de bourses de thèse est particulièrement important dans cet environnement. Comme indiqué précédemment le partenaire 2 procédera aussi à l'organisation d'un colloque.

Partenaire 3

Comme nous l'avons dit, un de nos buts est de favoriser l'organisation d'une communauté qui en train de se cristalliser. Certains membres important de la mouvance de l'inférence profonde sont à l'étranger, en particulier en Suisse, en Allemagne et en Angleterre. Il est naturel d'aider leurs étudiants (ainsi que les nôtres) à s'insérer dans la communauté scientifique. Deux bourses postdoctorales sont prévues à cet effet, une pour le Partenaire 3 et l'autre pour le Partenaire 1 (il va sans dire que nous pensons privilégier les candidats provenant de l'extérieur de notre projet).